PHILADELPHIA, MAY 26, 1877.

ORIGINAL LECTURES.

ASPIRATION IN COMPLICATED CASES OF PLEURAL EFFUSION.

BY WILLIAM PEPPER, M.D.,
Professor of Clinical Medicine in the University of Pennsylvania.

Reported by S. M. MILLER, M.D.

LECTURE I.—PYO-PNEUMOTHORAX.

IT is my intention in the present lecture, and in others to follow, to speak of the use of the aspirator in the treatment of difficult cases of pleural effusion. The case which I shall consider to-day has the following history:

Case I.—C. H., 24, single, clerk, was admitted to Philadelphia Hospital on March 30, 1876. Had lost sister and aunt by acute phthisis. His own health was good until last Christmas (1875), when after a severe wetting he had a light chill, followed by a slight cough with but little expectoration. The cough persisted without hæmoptysis, and with only gradual failure in general health, until May, when marked dyspnœa was superadded to the other symptoms. The patient first came under my care in October, 1876, when examination revealed the following condition: marked distention of left half of chest, the right semi-circumference measuring sixteen, the left eighteen inches on line of nipple. The mobility of the right chest in expansion was one and one-quarter inches; of the left only one-quarter inch. The apex-beat of the heart was in the fifth intercostal space, midway between the right border of the sternum and the line of the right nipple, but the impulse of the heart was extended and reached from this point up to the fourth interspace and out to beyond the nipple-line. There was also a distinct second centre of impulse in the third interspace, midway between the right nipple-line and the sternum. The percussion of the right chest was normal: over the left chest there was tympanitic resonance down to the third interspace, below which there was flatness anteriorly and posteriorly. The lower intercostal spaces bulged slightly, and the level of dulness changed by changing the position of the body: when lying on his back, there was tympanitic resonance over the whole anterior part of the left chest. On the right side there was exaggerated vesicular murmur. The left side showed entire absence of respiratory sounds from apex to base. Occasionally during respiration there was distinct metallic tinkling, which was heard most markedly over the third interspace anteriorly; cough developed intense succussion-splash.

The diagnosis was empyema with pneumothorax probably connected with tubercular disease of the lung. The patient had by this time lost considerable flesh, and suffered from hectic fever, with much dyspnœa and disturbance of cardiac action. On October 4, f℥lxxxiv (84) of thick, fetid pus were drawn off by the aspirating needle, introduced through the seventh interspace on a line with the anterior border of the axilla.

The operation was followed by marked relief: the distention of the left chest disappeared, and the apex-beat returned to the left of the sternum. It was evident, however, that the lung was incapable of expansion, and, though no air was admitted by the operation, the physical signs showed the existence of extensive pneumothorax, the pus which had been withdrawn being replaced by air or gas. In the course of the next six weeks the pus reaccumulated, and when it had reached the second interspace the serious symptoms of respiratory embarrassment returned. A fine drainage-tube was now passed round the seventh rib by means of a large curved needle, and was allowed to remain, one end coming out in the sixth and the other in the seventh interspace. A poultice was applied, and the pus was allowed to discharge. The patient was much relieved, but the discharge continued profuse and offensive, despite injections of various kinds. The cough grew worse, with purulent sputa, and signs of tubercular deposit appeared at the right apex. After a period of progressive debility, with anorexia, hectic fever, and emaciation, he died in December.

At the post-mortem examination the right lung presented scattered tubercles at the apex, which in several points had begun to soften; the lower lobe was still healthy. There were some slight pleural adhesions, but no effusion. The left pleural sac was distended with fetid gas, and contained f℥xxiv of thick, offensive pus. The left lung was greatly contracted from pressure, and on section showed a state of extreme carnification, with numerous tubercular granulations scattered throughout the tissue, some gray, others yellowish and softening. The pleura was much thickened, and both pulmonary and costal surfaces presented a shaggy appearance. Upon inflating the lung, air was found to escape at two points, where there was a minute ulcerative destruction of the pleura, due to softening of superficial, subpleural tubercles. The liver and kidneys were enlarged and fatty.

The recognition of the pyo-pneumothorax in this case was sufficiently easy. The tympanitic resonance over the left apex was distinguished from the pseudo-tympanitic resonance often heard over the upper lobe in cases of considerable pleural effusion, by its purely tympanitic character, and in addition there were the absence of

respiratory sounds and the occurrence of metallic tinkling. The presence of a large amount of pus free to move in the pleural cavity was easily detected by the distention and immobility of the side, the bulging of the intercostal spaces, the marked displacement of the heart, and the dulness varying with the changes in the position of the body.

A more difficult question was the determination of the source of the gas, as well as of the essential nature of the primary disease. The occurrence of putrefaction of a pleural effusion, with the development of gas, without any communication with the air, may perhaps be admitted as possible, but is at least so rare an event that it is safer to assume in any given case, as in the present, that the gas has been admitted to the pleural sac from without. The only way in which this could be effected here was by perforation of the pulmonary pleura. In order to arrive at a correct idea of how this occurred, it is necessary to turn to the history of the case, as well as to bear in mind the existence of a large empyema when the patient first came under notice. The family predisposition to phthisis, the occurrence of dry hacking cough continuing for several months, with gradual failure in health, naturally led to the suspicion that the attack began as one of phthisis of the left lung. The first thought—viz., that there had been a primary suppurative pleurisy which had led to perforation of the pulmonary pleura and to the development of pyo-pneumothorax—was disproved by the mode of attack, and in addition by the fact that at no time had there been such copious purulent expectoration as would have existed had free communication been established between a large empyema and the pulmonary tissue.

Pulmonary phthisis may become complicated with pyo-pneumothorax in two ways. A superficial vomica may rupture into the pleural sac, and air and pus gain entrance thereto, or a small sub-pleural tubercle may soften and lead to perforation of the pulmonary pleura, cause the escape of air, and thus induce a pleurisy which in the cachectic state of the system is likely to be attended with purulent formation. It is rarely that a superficial vomica exists without the occurrence of pleural adhesions which would prevent the entire collapse of the lung and the development of large pyo-pneumothorax, such as here existed. Moreover, the short duration of pulmonary symptoms before the appearance of the evidences of pleural effusion, and particularly the absence of purulent expectoration or hæmoptysis, went to show that there had not been a vomica of any size in this case. On the whole, therefore, it seemed most probable that the original condition had been one of disseminated tuberculous disease of the left lung; that a small sub-pleural nodule had softened and ulcerated through the pleura and allowed the escape of air, causing a pneumothorax; that subsequently pleurisy was developed, with the formation of a gradually increasing amount of pus. It will be seen that the post-mortem examination confirmed this view in every particular. The thick layers of false membrane which had been formed over the pulmonary and costal pleuræ partially covered the minute openings in the pleura, of which but two actually existed. While, therefore, some air was clearly able to escape from the vesicular structure of the lung into the pleural sac, as was shown by the increase in pneumothorax following the first paracentesis, there was no fistula through which the pus in the pleura could freely escape into the bronchial tubes and be expectorated.

A few words must suffice for the important questions that arose in connection with the treatment of this case. It may, I believe, be laid down as a general rule that, in pyo-pneumothorax connected with pulmonary phthisis, paracentesis should not be performed unless the collection increases to such an extent as to cause imminent danger. The reasons of this are to be found in the essentially incurable nature of the primary lesion; in the impossibility of preventing a reformation of the effusion; and, lastly, in the fact that when a lung, the seat of phthisis, becomes compressed by a pleural effusion, the progress of the pulmonary lesion is for the time usually checked. In the present case the absence of any careful examination of the patient in the early stage made it impossible to be certain that phthisis had existed, though the general considerations above given rendered it probable. It was, however, possible that the pleurisy had been primary, that it had caused a minute perforation into the lung-tissue, and that this had closed after allowing the entrance of a

certain amount of air to the pleura. Again, the very large amount of the effusion, and the serious interference it was causing with respiration and circulation, began to render interposition necessary. The operation was undertaken, therefore, as a tentative measure, in the hope that it would be followed by some expansion of the lung. The failure of this to occur confirmed the diagnosis that had been made, and rendered hopeless any future operative treatment. The drainage-tube was, however, introduced at the urgent request of the patient, in the hope that by maintaining a free discharge relief might be afforded to the distressing dyspnœa. The rapidly fatal progress of the case afterwards seemed to show that this was no exception to the general rule above stated, and that this last operation should probably have been declined.

ORIGINAL COMMUNICATIONS.

ON A NEW FUNCTION OF THE LIVER.

BY B. F. LAUTENBACH, M.D.

WHEN the portal vein of an animal is tied immediately before its entrance into the liver, the animal passes into a state resembling that following a fatal dose of one of the narcotics,—morphia, for example,—from which it differs, however, in that there is not a direct production of sleep, but an abolition of sensibility which predisposes to sleep. The animal remains in this state until its death, which occurs—always without convulsions—in the dog in from a half an hour to four hours, and in the rabbit and cat even earlier.

Usually after death the spleen is found enormously gorged with blood, as are also the walls of the stomach and intestines. The liver, on the contrary, is found to contain almost no blood, being much paler than normal. The gall-bladder is empty.

At times the engorgement is but slightly developed, or entirely absent, in which cases I have always found a communication of one of the veins of the portal circulation with one of those of the general circulation, this easily explaining why the stasis did not occur. In one experiment where this engorgement was not found, the vena parumbilicalis (Schiff, Sappey)—which exists in mammals throughout life—was very much enlarged, and a communication of it with one of the mesenteric veins was also very large, thus forming a new portal circulation.

In a series of experiments, a large number of which were made in connection with Professor Schiff, I tied the vena porta of mammals nearly sixty times, and always obtained the same symptoms. These symptoms are in brief the following:

A great tendency to sleep, owing to the abolition of both tactile and general sensibility.

A diminished frequency in the beat of the heart, and an increased, followed by a decreased, arterial pressure.

Paresis of the heart-arresting fibres of the pneumogastrics, so that irritation of the sympathico-vagi trunks will no longer cause cessation of the heart's movements. In this connection it must be stated that, following the ligature, contractions of the diaphragm occur isochronous with the beat of the heart, and disappear when the left phrenicus is lifted off the pericardium, to occur again when this is again brought in contact with the heart. I have frequently observed this symptom in animals that have been poisoned with drugs which produce paralysis of the heart-arrestors; and I have shown* that this phenomenon is due to an electrical current which is developed in the muscles of the heart and conducted by the left phrenic nerve (the right not being in contact with the pericardium, its presence or removal has no effect on the contractions) to the diaphragm.

A great diminution in the number of the respiratory movements, which frequently become stertorous.

Shortly before death a most curious symptom can frequently be observed. The animal has ceased to respire for several minutes, there is no heart-beat to be felt, and to all purposes the animal is dead: if, however, you now draw on the trunk of the carotid slightly downwards, the animal frequently commences again to breathe, and the heart-beat again becomes manifest, to cease again after several minutes. Should artificial respiration be resorted to, the beat of the heart would continue for an indefinite period of time.

In the cold-blooded animals a different result is obtained. I have extirpated the

* Phila. Med. Times, March 31, 1877, p. 296.

livers of more than a hundred frogs and found none of the characteristic symptoms which occur after tying the vena porta in mammals; but, as will be shown later, evidence enough was obtained to show that the system was no longer in its normal state. This is also indicated by the circumstance that in a number of experiments where the hepatic vena cava (vena porta) of frogs was tied, similar results to those following the same operation in mammals were obtained.

Having seen the effects that ligature of the vena porta produces, it remains now to seek their cause.

That they are not due to the accumulation of blood in the portal circulation and a consequent diminution in the quantity going to the other portions of the body was determined in the experiments where I tied the hepatic veins, which carry all the blood from the liver to the vena cava, and found that this produced absolutely no symptoms.

If death in these cases were due to anæmia, convulsions would invariably precede the fatal termination; yet I have never met with convulsions following this operation. The study of the arterial pressure also shows that this theory is not the true one. On watching the blood-pressure in the manometer it is found that after the ligature the column of mercury rises to nearly double the height it had before.

Nor do the symptoms occur when the biliary ducts are ligated; and consequently they cannot be explained on a theory of a retention of the biliary matters in the system.

Again, the symptoms cannot be due to the formation, through decomposition, of sepsin in the liver, as, (1) the formation of this poison occupies a much longer time than the time which expires after the application of the ligature up to the appearance of the symptoms; (2) the expressed juice, so to speak, of the liver of an animal that has been killed by tying its vena porta has absolutely no poisonous properties; (3) in the experiments where the vena porta was tied and by means of a tube blood was passed from the renal artery into the portion of the porta above the ligature, thus supplying the liver with blood enough to certainly prevent decomposition, the animals died even more rapidly than when this was not done.

It is hardly necessary to state that the ligature of the vena cava produces none of the characteristic symptoms which follow the vena porta ligature.

Having given and disproved some of the theories which may possibly be advanced to explain the interesting symptoms following this ligature, there remains to be given that which I believe to be their true explanation.

Certain animals—some of the snake species and many insects—secrete normally, and others—the dog and cat in the hydrophobic state—secrete under pathological conditions very virulent poisons, the presence of which in some animals and not in others has never been satisfactorily explained. May its explanation not be found in the results of the experiment to which I have just alluded? *i.e.*, may not all animals have a poison formed in their system, which were it not excreted, as in the snakes and other animals, or destroyed by certain organs, the animals would rapidly succumb under its influence?

In this paper an attempt will be made more especially to prove that the symptoms following ligation of the vena porta are due to the accumulation in the blood of a poison which under normal conditions is destroyed by the liver.

If such a poison exists in the blood of an animal in whom the vena porta has been tied, the injection of a small quantity of this blood, though of course it could have no effect on any of the higher animals,—the operated animal dying just as soon as a sufficient accumulation of the poison has taken place to produce a cessation of the vital processes, and no fractional portion of its blood could possibly have decided effect on another animal of its class,—might have some effect on the frog, and especially on the frog whose liver had been extirpated.

In thirty-four experiments I injected from one and a half to three cubic centimetres of blood taken from the vena porta, the vena cava, the vena jugularis, and from the right ventricles of dogs dying from ligature of the vena porta, into the lymph-sacs of the thighs of frogs, and these animals always exhibited symptoms analogous to those following the ligature of the vena porta in the higher animals, and almost always died within three hours.

As control-experiments, venous blood was always taken from the animal whose

vena porta was about to be ligated, and injected into liverless frogs, and never were there any symptoms to be observed.

Nor did the blood of animals in whom the vena cava was tied, or from animals in whom a sling was placed around the vena porta without its being ligated, produce the slightest effect on frogs.

The following are illustrative experiments:

Animal.	Injection.	Operation.	Time.	Symptoms.
Dog, 22 lbs.		A sling around the vena porta; not ligated.	3, 3, 1877. 4.20	
R. esculenta. R. temporaria.	2 c.c. of blood from the v. jugularis was injected into two frogs in whom the liver had been extirpated.		3, 4, 1877, 10.04	
			11.00	No symptoms.
			12.00	No symptoms.
			3.00	No symptoms have been produced in either the dog or the frogs.
		Vena porta ligated.	3.35	
			3.45	Animal has passed into a state of somnolence. Irritation of the pneumogastrics does not produce arrest of the heart-beat.
R. esculenta. R. temporaria.	2 c.c. of blood from the vena jugularis was now injected into two other frogs in whom the liver had been removed.		3.48	
			3.54	When placed on their backs the frogs are unable to re-attain the abdominal position. All the movements are incoordinate.
			4.22	The animals are dead.

The blood of an animal in whom the vena porta has been tied must therefore contain a violent poison which does not exist to an appreciable extent in the blood of normal animals, but accumulates just as soon as the vena porta is tied.

All attempts which I have made to isolate this poison have proved futile, owing to its great volatility or destructibility; heating the blood but slightly in a water-bath is frequently sufficient to cause it to lose its poisonous properties.

It will no doubt occur to the reader, as it did to the author, that the vena porta poison is not the only poison which can be destroyed in its passage through the liver. Having frequently observed in my experiments with nicotine that it is almost impossible to kill an animal by giving this drug by the mouth, it seemed plausible that this is such a poison.

NICOTINE.—Having determined that one drop of this poison injected into the general circulation would kill even a large-sized dog, the author in a number of dogs injected this poison so that it had to pass through the liver before it could enter the general circulation. This was done sometimes by injecting into the small intestine, and sometimes by injecting into the mesenteric and splenic veins.

The following are the results obtained by this method of experimentation:

One drop of nicotine injected into the mesenteric or splenic vein of a dog produces deep sonorous respiratory movements; an increased frequency and feebleness in the heart-pulse, so that at times it becomes imperceptible; retraction of the bulb of the eye, and a protrusion of the nictitating membrane; but the most characteristic symptom is the loss of sensation and especially the loss of the sense of touch. The animal in this respect resembles an animal in whom both the anterior hemispherical lobes of the brain have been extirpated; in walking it evidently does not feel the floor, as it frequently rests on the dorsal instead of on the plantar surface of its feet, and strong electrical irritation of the sciatic nerve does not dilate the pupil, this latter being the most delicate test to determine the absence of sensibility. Usually in fifteen minutes, but certainly in an hour, the animal has recovered, and acts like a normal dog. Even two drops of nicotine fail to kill a dog when injected into one of the veins of the portal circulation, or, what is really the same thing, into the intestine.

An injection of one drop of this drug into the subcutaneous connective tissue, or into a vein of the general circulation, produces all the symptoms just given as being present when the injection is made into one of the veins of the portal circulation;

but, in addition, intense prostration, trismus, general tetanus, and death frequently occur within one minute after the injection.

These results will be best seen in the following experiment:

Animal.	Injection.	Symptoms.
Dog, 10 lbs.		Respirations, 18–21. Pulse, 120. Temperature, 39°.60 (C.)
Time.		
11.55	Nicotine one drop into a mesenteric vein.	In a very short time the pulse rose to 204; the respirations became sonorous, the temperature descended to 37°.97 (C.) Retraction of the bulb of the eye and protrusion of the third membrane. On walking, the dog shows symptoms of a loss of the tactile sense; *i.e.*, the animal drags its feet, and frequently sets them down on their dorsal instead of on their plantar surface.
12.10		Respirations very strong; pulse 156, feeble, and felt only during inspirations.
12.35		Pulse 150; respirations 22; pupil small and reacts to light.
12.40		Pulse 23, resp. 20.
1.00		The dog has completely recovered.
1.50	Nicotine one drop into a mesenteric vein.	Immediately after the injection,—pulse 37, 36, 25, 22, 19, 15, in ten seconds; loss of tactile sense; respirations sonorous. Nictitating membrane protruded.
2.40		The dog has again recovered.
5.07	Nicotine one drop into a subcutaneous vein of the posterior extremity.	Tetanus; trismus; pulse 210; respiration difficult. Death in one minute.

In the normal dog one-half drop of nicotine never produces death; but in dogs whose vena porta has been tied the author found that trismus and tetanus were produced, and death usually rapidly occurred, when but one-fifth drop was injected.

In normal frogs of the esculenta and temporaria species, an injection of one-tenth drop of nicotine into the general circulation always caused death under similar symptoms to those occurring in mammals. One-twentieth drop always produced marked symptoms, but never death. In frogs, however, whose livers had been extirpated, even one-fortieth drop produced death, as will be seen in the following experiment:

Animal.	Injection.	Operation.	Symptoms.
R. esculenta. Feb. 1, 1877.	Nicotine one-twentieth drop into the lymph-sac of the thigh.		
Feb. 2, 9.14 A.M.		Extirpation of the whole liver.	The animal was slightly ill, but soon recovered.
10.00	Nicotine one-thirtieth drop into the lymph-sac of the thigh.		
10.04			All voluntary movements have ceased. Pulse reduced from 48 to 27. Paralysis of the tactile sense.
10.16			The frog is dead. Nerves and muscles respond to irritation.

How else can these experiments be explained except on the theory that the presence of the liver is necessary for the destruction of the poison? The word destruction, and not elimination, is employed, because the author found that tying the gall-ducts of the frog did not in the least influence the quantity of the poison necessary to produce death.

In a large number of experiments one-fortieth drop of nicotine was injected into normal frogs without producing any symptoms whatsoever. Four hours after the injection the entire livers of the animals were removed, and twenty-four hours later one-fortieth drop was again injected, when, under symptoms of nicotine-poisoning, the animals died.

Against these latter experiments it cannot be said that possibly a second dose is more poisonous than the first, as I always found in unoperated animals the symptoms following a second much less marked than those following the first injection.

One-fifth drop of the drug injected into the intestine of a frog produced slight symptoms of loss of sensibility, but if now the liver of the frog was removed, and a day later the same dose again injected into the intestine, rapidly fatal symptoms were produced.

In the next series of experiments my object was to see if the fatal dose remained the same when the activity of the liver was increased by being placed in a hyper-

æmic state. To make the liver hyperæmic the author availed himself of an anatomical peculiarity in the frog. In the frog the liver does not receive the whole venous blood of the abdomen: only a portion of it is carried to the organ by the hepatic vena cava, while another portion is conveyed directly to the heart by a second vena cava without passing through the liver. To produce hyperæmia of the liver it is sufficient to force all the venous blood of the abdomen to traverse the liver; and that was accomplished by tying the second or superior vena cava.

When now one-tenth drop—the fatal dose for a frog—was injected, not the slightest symptom was observed, nor would one-fifth drop produce death. From this we see that a greater activity of the liver causes a larger quantity of the poison to be destroyed.

The following experiment is characteristic of this series of experiments:

Animal.	Injection.	Operation.	Symptoms.
R. esculenta.		Ligature of second v. cava.	
Time.			
9.17			
11.00	Nicotine one-tenth drop into the lymph-sac of the back.		
1.25			No symptoms have occurred up to this time.
2.00			No symptoms
3.00			No symptoms

The following experiment shows that in the normal frog one-tenth drop of nicotine produces death:

Animal.	Injection.	Symptoms.
R. esculenta.	Nicotine one-tenth drop into lymph-sac of the back.	
Time.		
9.03		Slowing of the pulse, abolition of the tactile sense, and absence of voluntary movements.
9.31		Death.

Thinking it possible that the mere intimate contact with the liver-substance would be sufficient to cause the nicotine to lose its poisonous properties, the author made a large number of experiments by macerating the livers of dogs and rabbits with from three to five drops of nicotine, the expressed juice of the macerated mass then being injected into dogs and frogs. As will be seen in the following experiments, the fatal symptoms were, under these circumstances, never produced:

(1). "The liver of a newly-killed rabbit was taken and macerated with ten cubic centimetres of water and three drops of nicotine. The expressed juice of this mass was then injected into the cellular tissue of the posterior extremity of a dog weighing eleven pounds.

"In a short time the dog showed the respiratory symptoms; the protrusion of the third conjunctival membrane; dilatation of the pupil and the loss of the tactile sense; but none of the alarming narcotic and tetanic symptoms occurred; and in several hours the dog had recovered."

(2). "The liver of a dog who had died forty-eight minutes after his vena porta had been tied, was macerated with three drops of nicotine, and one-third of the expressed juice was injected into each of three liverless frogs.

"After thirty minutes, slight symptoms of nicotine-poisoning appeared, but these symptoms soon passed off."

After having found that the expressed juice of the kidney itself is not poisonous, the author as control-experiments macerated the kidneys of healthy animals with but two drops of nicotine, and the expressed juice produced the death of the animals into whom it was injected.

From these facts the author feels justified in believing that nicotine contains two poisons, one of which produces the non-fatal symptoms which so closely resemble those of locomotor ataxia. This poison is not at all affected by the liver. The other poison producing the tetanic symptoms and rapid death is entirely destroyed in passing through the liver.

Another explanation which we may get from these experiments is of the comparative absence of danger which attends the bringing of tobacco into the stomach through tobacco-chewing, as compared to the danger of inhaling the fumes of tobacco into the lungs. In the latter case the nicotine directly enters the general circulation, while in the former, owing to its having to pass through the liver, but little of the poison can enter the circulation.

HYOSCYAMIA.—In examining the other poisons for one which was wholly destroyed

in passing through the liver, hyoscyamia was found to be such a one. One-half to three-quarters of a grain of this alkaloid will always kill a dog when introduced into the general circulation, under symptoms of diminution in the frequency of the pulse; paralysis of the sense of touch; paralysis of the heart-arresting fibres of the pneumogastrics; great somnolence, and even coma. In the experiments where one grain was injected into the portal circulation of dogs, absolutely no symptoms were produced; *i.e.*, a more than fatal dose had no effect when forced to pass through the liver before entering the general circulation.

The expressed juice from the livers of dogs, macerated with two to five grains of hyoscyamia, was injected into the general circulation of dogs and frogs, and no symptoms occurred. When, however, the kidney or spleen of a dog is macerated with but one grain of hyoscyamia, and the expressed juice is injected into dogs and frogs, these animals die under symptoms of hyoscyamia-poisoning.

In the frog one-twenty-fifth drop produces death under symptoms similar to those produced by the drug in the higher animals, with the addition of a very marked and peculiar slowing of all the movements of the animal. Following irritation of the nerves a longer time elapses before the muscles commence to contract, and when contracted they remain so longer than the contraction following the irritation of the nerves of an animal not poisoned.

One-fortieth drop produces but slight symptoms in the frog. If however, the same frog was taken twenty-four hours later and the liver extirpated and then but one-sixtieth drop of the drug injected, the animal showed very marked symptoms and died, thus showing again that the absence of the liver causes a much smaller dose of the drug to be poisonous than when this organ is present.

Hyoscyamia produces one symptom in the liverless frog which it does not produce in the normal frog. If we examine a frog who has been poisoned by hyoscyamia after the liver had been removed, we find that reflex movements continue until near death; but if we attempt to irritate the sciatic nerve we find the strongest electrical currents will not produce contraction of the muscles. That is, a nerve which is still able to conduct the impulse necessary to produce a reflex movement is itself not excitable by direct irritation. The explanation of this result will be found in the difference between the receiving and conducting powers of the nerves to which I have recently drawn attention.*

Some of the experiments which I made gave such strange results that it seemed as if some source of fallacy must exist. Carefully repeating these experiments again and again, and always obtaining the same results, convinced me that all doubts were unfounded. These experiments are so interesting that one will be given in full.

"At twenty minutes before two, one-sixtieth drop of hyoscyamia was injected into the lymph-sac of the thigh of a medium-sized frog of the esculenta variety. Slight slowing of the movements was the only symptom. At 4.00, the animal was as before the injection. At 4.55 its liver was extirpated, and at 4.58—but three minutes later—the animal showed great slowness in all its movements. When the posterior extremities were extended, in response to a slight irritation, it took 8″ for them to return to their normal position. When the animal was placed on its back it could not re-attain its abdominal position. In one hour the animal was perfectly motionless, and in three hours its heart had ceased to beat. After death, irritation of the nerves no longer produced contractions of the muscles; but direct irritation of the latter caused their contraction."

In this experiment the injection of the poison produced very slight symptoms, which disappeared after several hours. One hour after their disappearance the liver of the frog was removed, when immediately, and without any more of the poison being given, the animal showed intense symptoms of hyoscyamia-poisoning, followed by death.

For some time I was at a loss for a plausible theory to explain the results obtained in the experiments similar to the one just given. The hyoscyamia was circulating in the blood, and reaching all the organs which it could affect, in no greater quantity after than before the extirpation of the liver, and consequently it alone could not have produced these marked symptoms. There must have been another poison formed in the frog, which, combined with the small

* Philadelphia Medical Times, vol. vii. No. 243.

quantity of hyoscyamia still in the blood, produced these fatal symptoms. This poison could have been none other than the poison which I spoke of in mammals as the vena porta poison, thus showing that, though extirpation of the liver in frogs does not produce manifest symptoms, yet a poison is thereafter retained in the system which would have been removed by the liver had not this organ been extirpated.

We have seen from the previous experiments that neither of these two poisons alone—*i.e.*, neither the extirpation of the liver nor the injection of one-sixtieth drop of hyoscyamia—could produce this result in frogs, but a combination of the two, before the system had time to become accustomed to the presence of the "vena porta" poison, was necessary to produce the fatal result.

The great similarity in the effects of these two poisons is remarkable.

Hyoscyamia slows the movements, as also does the vena porta poison.

They both modify sensation.

They both produce a tendency to sleep.

They both paralyze the heart-arresting fibres of the pneumogastrics.

With either alone the nerves remain excitable until long after death, but with a combination of the two the nerves are still able to conduct impressions from the centres to the periphery, and thereby produce movements, but it is impossible to cause the nerves to conduct impressions applied to their trunks.

In the author's further experiments with hyoscyamia it was found that, while one-twenty-fifth of a drop was fatal to normal frogs, one fifteenth did not produce dangerous symptoms in frogs whose livers had been rendered hyperæmic by tying the second vena cava.

In hyoscyamia we have, therefore, an organic poison which is destroyed by the liver, as the author's experiments will certainly allow of no other conclusion.

CONIA is another drug whose active properties the author found to be destroyed by the liver. As the experiments are not essentially different from those made with hyoscyamia, it will not be necessary to infringe on the reader's time and patience by giving them.

Recently, through the kindness of Dr. Mitchell, the author obtained a small quantity of the *venom of the cobra-snake*, with which a new series of experiments were made.

A fatal dose of this poison injected into the mesenteric vein of a cat produces no symptoms, although the same dose kills the same cat if introduced into the general circulation some hours later.

A dose which had produced no effect on a frog killed this frog when its liver had been removed.

A fatal dose was no longer fatal when the liver of the frog was made hyperæmic by tying the second vena cava.

The venom of the cobra snake is, therefore, also a poison whose active properties are destroyed by the liver. This is extremely interesting, as it will explain why the animal poisons when swallowed produce no symptoms.

Among the drugs which the author has found not to have their effect modified by the liver are curare, prussic acid, and atropia. As regards the latter, some doubts may be expressed, as it and hyoscyamia are frequently considered to be one and the same alkaloid; but this is not so, as the two differ in a number of respects as regards their physiological action.

The experiments which have been given show distinctly that some of the organic poisons are destroyed in passing through the liver, and it is probable that the number of poisons thus destroyed will not prove to be inconsiderable, it being a well-known fact that many of the alkaloids do not have the same effect when given by the mouth as when given hypodermically. By the former method the greater portion of the drug has to pass through the liver on its way to the general circulation, while by the latter all enters directly into the general circulation. It must not, however, be thought that every drug which does not produce its effects when given by the stomach is destroyed by the liver; as it is possible that they are not absorbed from the alimentary canal. This the author has found to be the cause of the non-action of curare and of the but partial action of atropine when thus given.

Before closing this article there is still one symptom to speak of that frequently follows the extirpation of the liver in frogs, which shows that, though this operation produces no symptoms which are apparent at first, nevertheless a poison is developed and operative.

Goltz,* nine years ago, found that when

* Functionen d. Nervencentren des Frosches, p. 1.

he removed the cerebrum of frogs and rubbed a smooth object along the back of the animal, or when he merely laid the animal on its back, it croaks each time that its back is rubbed, so that it would not be difficult to have the frogs of Aristophanes: any physiologist could with very little trouble have a frog chorus that would, to the amusement of the public, let their voices be heard when wished for. I have found that extirpation of the cerebrum is not necessary for the production of this symptom, but that the section of one of the pedunculi cerebri is all-sufficient. In the unoperated frog this croaking cannot be produced.

Goltz explains this experiment by saying that the rubbing excites reflexly the centres of the voice existing in the medulla oblongata, and when this is no longer influenced by the cerebrum, croaking will result; but when the cerebrum is present, this acts as an arrestor of reflex action, and consequently the croaking does not occur.

Many frogs who have been liverless for some days, when placed or when rubbed on their back croak even more constantly than a frog whose cerebrum has been removed. Of course no one would from this experiment be likely to infer that the liver is an arrestor of reflex action, although its removal causes an arrest on the reflex activity of the voice to be removed. But how then is the result obtained to be explained? The same symptom is to be observed in frogs poisoned with hyoscyamia. In these animals it must be due to some modifying influence which the drug has on the sensory-motor tracts of the brain, of which the fibres pass through the pedunculus on their way to the great gangliæ.

The same explanation must be given for the symptom occurring in liverless frogs, *i.e.*, a poison is also accumulated in these animals which, after several days, paralyzes the arresting influence which the cerebrum has on the reflex activity of the voice.

The experiments alluded to in the preceding paper have brought the author to the following conclusions:

1. *The liver has for one of its functions the office of destroying certain of the organic poisons.*

2. *A poison is being constantly formed in the system of every animal which it is the office of the liver to destroy.*

The experiments—two hundred and eighty-three in number—forming the basis of the foregoing paper were almost all made in the Physiological Laboratory of Professor Schiff, at Geneva, for whose kind assistance and advice the author most gratefully acknowledges his obligations.

ON THE DIAGNOSIS OF URETHRAL STRICTURE BY BULBOUS BOUGIES.

WITH ILLUSTRATIVE CASES.

BY J. WILLIAM WHITE, M.D.,

Lecturer on Venereal Diseases in the University of Pennsylvania, Surgeon to the Philadelphia Hospital, etc.

DURING the last few years the methods of investigating the male urethra have undergone a decided change. It is no longer generally believed that the easy introduction into the bladder of a sound of twenty or twenty-one millimetres in circumference, or indeed of any fixed size, is sufficient to demonstrate the non-existence of organic stricture.* The great variability in calibre of different urethræ has been demonstrated, and has of course modified the methods used for exploration.

Strictures of extreme calibre have been recognized, both during life and at autopsies, and have been assigned their true position as important agents in the production of cystic and renal diseases and of many reflex disturbances of the most alarming character.

Contraction of the meatus urinarius has been found to be at once a more frequent and more serious pathological condition than was previously suspected,† and the absence in many instances of any definite

* Sir Henry Thompson is probably the most distinguished living surgeon who is not included in this assertion. His last utterance on the subject, so far as I am aware, was published in the London *Lancet* of December 11, 1875. He then said, "When a young man consults you for certain troubles, relative to which you desire to learn whether urethral obstruction be a cause or not, do not be tempted for an instant to adopt so unnecessary a course (to say the least) as the introduction of very large instruments or instruments with huge bulbs at the end of them. But simply take a flexible English gum-elastic bougie, well curved towards the point, with a blunt end, not larger, as a rule, than No. 10 or 11 of our scale, and pass it very gently and slowly into the bladder. If it goes easily,—above all, if it is withdrawn without being held, and slides out with perfect facility,—take my word for it, he has no stricture, and, *quoad* obstruction, wants no use of instruments whatever." As will be seen, however, by a foot-note on the following page, he testifies elsewhere to the great usefulness of the bulbous instruments in the determination of the existence of "any narrowing in any part of the urethra," an assertion which seems to me as much too absolute in one direction as is that just quoted in another. I infer from the context of his lectures on the urinary organs that he refers in this last remark rather to the locating of strictures already discovered than to their original detection.

† Mr. Furneaux Jordan, the London *Lancet*, January 29, 1876.

relation between this orifice and the rest of the urethra has been conclusively shown.*

As a result of these modifications of opinion, conical steel sounds, as explorers in cases of suspected stricture, have been largely supplemented if not supplanted by the flexible bulbous bougies of Leroy or the so-called *bougies à boule*. There can remain no reasonable doubt as to the great value of these instruments in detecting, locating, and measuring organic strictures of the urethra, and there is but one opinion expressed on this point by the majority of surgeons who have had their attention specially directed to urethral diseases.† In spite, however, of the unanimity of opinion among specialists as to this point, many valuable text-books of surgery entirely omit to mention these bougies, and many experienced practitioners neither recommend nor employ them.

It is in view of this fact that the subjoined brief extracts are presented, selections having been made from writers who cannot be accused of any special bias or violent partisanship in this connection. They might be multiplied indefinitely, but will suffice to indicate the estimate in which these instruments are held by those who have the most extended opportunities for their use.

A case which has recently occurred in my practice, and to which the subsequent post-mortem examination gives unusual interest, will probably be the best means of illustrating the value of bulbous bougies and the pathological importance of those strictures which cannot be detected without their employment, *i.e.*, strictures of extreme calibre.

A muscular, apparently robust man, æt. about 55 years, was admitted to my wards in the Philadelphia Hospital, suffering from a leg-ulcer. No urinary trouble was suspected until he was subjected to an examination for the purpose of verifying a theory to which I shall presently allude. He was then found to have a coarctation at the distance of four and a half inches from the meatus. There were no marked subjective symptoms whatever: micturition was possibly a little too frequent, and was not quite complete, a slight dribbling following the flow of urine, there were obscure lumbar and hypogastric pains, and occasional neuralgia of the glans penis; but these facts were elicited only after careful questioning. *A No.* 28 *steel sound passed into the bladder without the slightest perceptible difficulty*, but the patient complained of pain extending from the peno-scrotal junction to the perineum. An examination with a bulbous bougie, No. 24, revealed a stricture situated in the spongy portion of the urethra at the point mentioned, about four and a half inches from the meatus; No. 26, bulbous, could only be passed with great trouble.‡

Dilatation was commenced, but an attack of some intercurrent disease—pneumonia, I believe—caused the patient to be transferred by one of my internes to the medical wards, where he soon after died. At the autopsy I carefully removed the genito-urinary organs, on the appearance of which I have the following notes. The penis was of average size, the pendulous portion measuring about four and three-quarter inches in length and a little over three inches in circumference. (This would have fixed the normal calibre of the urethra, according to Dr. Otis's standard, at about thirty-one millimetres.§) Upon dividing it along the dorsum a band of fibrous structure was seen almost encircling the urethra four and a half inches from the meatus; extending from a point a little anterior to this back to the bulb of the corpus spongiosum and to the crura of the corpora cavernosa was an area of intense congestion, which persisted even after maceration of the specimen in chloral solution for two weeks and was then plainly noticeable across the room at a class demonstration; bladder dilated and hypertrophied; meatus slightly dilated; kidneys healthy; no enlargement of the prostate. Two small patches of ulceration existed on the

* Dr. F. N. Otis, *New York Medical Journal*, April, 1876.

† "With the bulbous bougies of various sizes you may determine the existence of any narrowing in any part of the urethra with great accuracy, and for many years I have never operated without employing them carefully beforehand."—Sir Henry Thompson, "On the Urinary Organs," p. 49; "Stricture of the Urethra," p. 148.

"A far better and more accurate means of exploration, and the only one which conveys any reliable idea of the extent and multiplicity of strictures, is the soft exploratory bulbous bougie."—Gross, "On the Urinary Organs," p. 462.

"The French *bougies à boule* afford the only means available (aside from the endoscope) of detecting slight urethral contractions."—Bumstead on "Venereal Diseases," p. 270.

"The bulbous bougie is an instrument essentially necessary for the diagnosis of stricture."—Van Buren and Keyes on "Genito-Urinary Diseases," p. 107.

"The urethra should be carefully examined by a sound with an olivary point, for stricture is far more readily detected by such an instrument than by an ordinary sound."—Berkeley Hill, "On Venereal Diseases," p. 369.

"Although the *bougies à boule* have some inconveniences, and can furnish only incomplete information, they still remain the best instrument we possess for exploring the urethra."—M. Voillemier, "Traité des Maladies des Voies urinaires," Paris, 1868.

‡ It may be remarked, *en passant*, that the weight of the steel instruments and their very gradual increase in diameter from the point to the shaft give them a dilating power so much superior to that of the flexible bougies that a difference of from four to six numbers almost invariably exists. That is, a stricture not markedly resilient or elastic, which will admit with slight resistance a bulb twenty-four millimetres in circumference, will at the same sitting, and with no greater apparent resistance, permit the introduction of a No. 28 or No. 30 steel sound. This quality, which gives these latter instruments such value in the treatment by gradual dilatation, necessarily lessens their delicacy as explorers, and accounts for their frequent failure to detect strictures of large calibre.

§ On Stricture of the Male Urethra, its Radical Cure. By Fessenden N. Otis, M.D. Pamphlet, pp. 36. New York, 1875.

floor of the membranous urethra, each measuring about two lines in diameter. A microscopical examination of the constricting band was made by Dr. Shakespeare, Pathologist to the Philadelphia Hospital, with the following result:

"The section which I made of the stricture submitted for microscopic examination by Dr. White included the mucous and submucous tissue. Longitudinal sections were made after the specimen had been hardened in alcohol. These, after staining in carmine and mounting in damar, exhibited, under the microscope, a great increase of the fibrous portions of the submucous tissue. Among the fibrous bundles were numerous typical, curly, elastic fibres. It was also noticeable that the walls of the capillary vessels running towards the mucous surface were very thick and fibrous (sclerosed). The inflammatory processes were in the last stages, as was evidenced by the sparseness of cellular elements scattered throughout the fibrous portion."

This case is, in my opinion, one of a large class formerly often overlooked, and still neglected or misunderstood by those practitioners who adhere to the old methods of examination. Its further progress, had it not been interrupted by acute disease and death, can be easily stated. In consequence of the imperfect emptying of the urinary tract, cystitis would sooner or later have resulted, and would probably have been regarded as the primary disease if the urethral coarctation had remained unrecognized. Pyelitis, or infiltration of urine, or both, would have followed, and would possibly have caused death by uræmic or pyæmic poisoning. It is of course admitted that the case is in many ways an unusual one. With the same amount of existing disease the subjective symptoms would ordinarily be much more marked, or the contraction giving rise to it would be of smaller calibre: actual ulceration is rare in these conditions, and an involvement of the urinary tract posterior to the bladder was hardly to be expected where there was so little obstacle to the flow of urine. The facts remain, however, that these are simply exaggerations of the pathological processes which beyond a doubt result from any infringement, however slight, upon the normal calibre of the urethra, and that an accurate diagnosis could not have been made and consequently rational treatment could not have been applied without the use of the bulbous or exploratory bougie; and this is beyond anything else the practical point to be derived from a study of this case.

While my experience in this and many similar cases, and my respect for the opinions of those whom I have quoted, lead me to the belief that the *bougie à boule* is an indispensable instrument to the surgeon who deals with urethral diseases, I recognize distinctly in connection with its use several sources of possible error. My attention having been called to the subject by the large number of deep-seated strictures which were discovered by a professional friend while pursuing an investigation into the results of masturbation, and my own experience among onanists having led me to a contrary conclusion, I began a series of observations upon healthy urethræ. Since then I have examined, in the out-patient department of the University and in the wards of the Philadelphia Hospital, a large number of individuals, probably a hundred, who were not conscious of any genito-urinary trouble whatever. I have not failed in a single instance to obtain, at a point varying from five and a half to seven and a half inches from the meatus, the characteristic arrest of the instrument and the subsequent jolt or jump which are so familiar to those who have used it in cases of stricture, the sensation being that of pulling a knotted rope through a relatively small aperture. It was evident at once that this condition could not be an abnormal one; and its explanation was sought, and I believe found, in the anatomy of the curved portion of the urethra. Any extended consideration of this would be inconsistent with the limits and purpose of this paper; but there are one or two points which seem to me worthy of special mention. Directly beneath the symphysis pubis the urethra perforates the posterior or visceral layer of the deep perineal fascia. This layer is properly a portion of the pelvic fascia. It fills in the sub-pubic arch from behind and passes along the sides of the membranous portion of the urethra to the prostate. A little in advance of this point is the anterior or integumental layer of the same fascia, continued round the under surface of the urethra, becoming lost upon the bulb and filling in the sub-pubic arch from in front. Between these two layers, which are often described together as the triangular ligament, are found various vascular structures, important muscles, Cowper's glands, and the membranous urethra. It is said that the anterior layer exerts the larger share in stopping

the progress of an instrument inwards, owing to the fact that just in front of it is the freely movable and dilatable bulbous portion of the urethra, permitting considerable motion in the point of the sound, which is suddenly arrested at the commencement of the membranous urethra by this firm fibrous anterior layer.* This is probably true; but it may be capable of demonstration that the posterior layer, for analogous reasons, offers a similar opposition to the free withdrawal of a bulbous bougie. The prostatic urethra being at once more movable and more dilatable than the membranous portion, the bulb slips smoothly along it until the point is reached at which this layer of fascia closely embraces the posterior part of the membranous urethra and the outer surface of the prostate. Here, for obvious reasons, it is arrested, and it is at this moment that the deceptive sensation which may be considered indicative of the existence of organic stricture is communicated to the hand.

A series of observations and dissections upon the cadaver has confirmed me in this view, which was originally purely theoretical, and has moreover eliminated the possibility of the resistance being due to a spasm of the compressor urethræ muscle which surrounds the canal at this point, arrest of the instrument occurring just as invariably after death as before.

Having in a number of cases carefully brought the bulb closely up to the point of resistance, an assistant has held it in position while I exposed it by a dissection of the deep urethra. I have always found the shoulder of the bulb in the exact locality of the deep layer of fascia,—the edge of which could often be felt tense and cord-like over the upper wall of the urethra. A division on either side of its attachment to the ramus of the ischium or pubes beneath the crura penis would then cause an immediate disappearance of the resistance, and the bulb could be drawn outwards smoothly and uninterruptedly. If the handle of the instrument were depressed during its withdrawal, the edge of the prostate became a cause of obstruction; and it may act thus to a greater or less degree in all cases.

It is not claimed that these observations are new, and it is of course well known that the region of the perineal fascia has long been considered one of the normal constrictions of the canal, and that frequent allusion has been made to this fascia as an obstacle to the passage of urethral instruments; but the subject does not seem to have been definitely discussed in its relation to bulbous bougies, and very distinguished surgeons have been apparently forgetful of it.†

In an able and important paper on "Sexual Debility and Impotence," published in the *Medical and Surgical Reporter* for May 5, 1877, Dr. S. W. Gross remarks that the majority of the cases of reduced sexual power which have come under his observation have been due to subacute inflammation and morbid sensibility of the curved portion of the urethra, and "were always associated with deep-seated strictures, usually of large calibre." He gives the results of his examination of nineteen such cases, attributing four of them to gonorrhœa, the rest to masturbation. He says, "It is interesting to note that the strictures, whether they were the effect

* Sir Henry Thompson, "Stricture of the Urethra," p. 58. Dr. Otis, whose opinions on genito-urinary disease are always worthy of careful consideration, has said in a recent article (*N. Y. Med. Journ.*, April, 1876) that "perfect security against mistaking a normal narrowing for stricture may always be had by examining from before backward. If the canal anterior to the contraction is of distinctly larger calibre, this localized contraction must be accepted as a stricture." I suppose he refers to the ante-bulbous urethra, though he does not distinctly say so. The anterior layer of the deep perineal fascia certainly offers in many cases a decided resistance to the introduction of a bulb, though it is not often so plainly perceptible during the withdrawal of the instrument. If Dr. Otis's rule were to be strictly applied, this point might often be wrongfully considered to be the seat of abnormal narrowing.

† Mr. B. Wills Richardson, in the *Dublin Journal of Medical Science* for November, 1873, remarks that he believes the bulbous bougie to be a deceptive urethral explorer, but says that for ordinary exploration it is preferable to a metallic sound. He fails to give any anatomical reasons for his belief in the fallacy of the bulbs.

Dr. Robert F. Weir, in an excellent article on "The Normal Urethra and its Constrictions" (*N. Y. Med. Journ.*, April, 1876), asserts that "our heretofore relied-upon methods of exploration are not sufficiently trustworthy for us to distinguish between normal and abnormal narrowings of the urethra, certainly from No. 29 upward," making the remark especially as a contradiction to the statement of Dr. Otis that "complete freedom from stricture can only be demonstrated by the easy passage of a bulbous sound of a size fully equal to the normal calibre of the urethra." He, however, alludes to occasional apparent constrictions or duplicatures of mucous membrane existing in the spongy portion of the urethra, and not to any deeper-seated and invariable trouble. Dr. H. B. Sands, likewise in the same journal, says that the deceptive sensation may arise from "a deviation of the sound from the axis of the canal, a spasmodic contraction of the muscular fibres which surround the urethra, or a puckering of its mucous membrane before the instrument." Both these gentlemen have confirmed by casts of the urethra the variations in size and dilatability of its different portions which Sir Everard Home, Ducamp, Reybard, and others had long ago shown to exist. The constrictions which Dr. Weir detected with the *bougies à boule* in the cadaver, and the existence of which subsequent post-mortem examination failed to confirm, may have been due to differences of elasticity in the spongy urethra. Civiale long ago observed that "from the fossa navicularis to the arch of the pubis the dilatability of the urethra does not sensibly vary, but that in the middle of this free portion there is a diminution of elasticity" (Traité pratique sur les Maladies des Organes Génito-Urinaires, 2d ed., Paris, 1850, p. 36). It was exactly at this locality—from two to three and a half inches from the meatus—that all of Dr. Weir's constrictions were found; and, as elasticity and dilatability are not interchangeable terms, this may explain not only the resistance to the passage of bougies, but also the fact that no constrictions were found in the casts at these points.

of onanism or of gonorrhœa, were usually single, deep-seated, and of large calibre;" and adds that he is "forced to adopt the view that urethral stricture, as well as hyperæsthesia, is an essential lesion of masturbation." He believes his measurements to be of practical importance, since they indicate that "the lesion is generally to be found at the sub-pubic curvature and its vicinity," and says that for its detection, instead of the ordinary instrument, he "would earnestly recommend the soft, exploratory, bulbous bougie."

The relation of my observations to those of Dr. Gross is evident, and it seems probable that in many of his cases he has mistaken the normal constriction to which allusion has been made, for an abnormal narrowing or stricture. Fortunately, this mistake, if it be one, does not seriously affect the practical value of his paper as regards the treatment recommended for the relief of these conditions. He says, "Of topical measures none has afforded me such good results as the introduction of the conical steel bougie, at first every forty-eight hours, and afterwards every day."

There can be no doubt of the benefit of this plan of treatment in such cases; but it is possible that its usefulness is due, not to the dilatation of an abnormal contraction, but to the sedative and resolvent effect usually following the judicious introduction of full-sized urethral instruments.* When, however, he adds that dilatation alone often fails, and that then "to effect a permanent cure an operation will be required," and advises internal urethrotomy, I dissent because in the majority of instances his incision would necessarily be made in that portion of the urethra which most seriously resents interference, and would be through the normal structure of the parts, and not through a constricting band of cicatricial material.

Moreover, I cannot accept his conclusion that "urethral stricture is an essential lesion of masturbation." A year or two ago, while acting as medical officer to the Eastern Penitentiary, a prison containing about a thousand convicts and conducted on the principle of separate confinement, I began some observations on the urethræ of those prisoners who were known to be persistent masturbators. I used both the steel sounds and the bulbous bougies in these cases, but abandoned the investigation on account of the impossibility of eliminating other causes of disease, —particularly gonorrhœa. I am sure, however, that, exclusive of those which I believed to be normal, I observed no large proportion of coarctations; and I do not think masturbation a sufficiently active or efficient cause for their frequent production. If it were such a cause, the effects would surely in a certain proportion of cases be more serious and more noticeable; if it were capable in nearly every case of producing one stricture of large calibre, it would sometimes be followed by several strictures of smaller calibre. The institution alluded to is one which affords exceptional advantages for the study and observation of onanism and its effects, and the subject was one in which I was specially interested; but I do not remember, during an experience of three years and a half, to have ever seen in a confirmed masturbator retention or incontinence of urine, frequent or difficult micturition, or any of the more marked symptoms which would usually accompany stricture of the urethra. Neither have I learned that those physicians who have charge of the insane, and who see large numbers of onanists, have ever made similar observations; although I freely admit the great probability of many such troubles having escaped notice.

The importance of Dr. Gross's paper, however, as establishing from actual experience the value of steel sounds as curative agents in cases of sexual debility and impotence, and as calling attention to the relation between these conditions and masturbation and organic stricture, cannot well be over-estimated, even though it be only in exceptional cases that this relation is as definite as is claimed by the distinguished writer.

One of the great advantages claimed for exploratory bougies, and one which to a certain extent properly belongs to them, is the power which they confer upon the patient himself of recognizing, equally with the surgeon, any abnormal contraction of the urethra; but I am sure, from repeated experiments on myself and on patients, that it is often impossible to distinguish, so far as the sense of resistance is concerned, between this normal obstacle and a deep-seated stricture; and the abuses which may arise from a knowledge of this fact by charlatans are almost as much to be dreaded as the errors of those who are unfamiliar with it.† It may be claimed that extended experience confers upon the operator the ability to distinguish by the tactile sense the difference between a normal and an abnormal contraction; but I do not think this is true. The problem is largely mechanical: the conditions are in every way analogous, and the resulting sensations both of the surgeon and the patient are almost precisely similar, varying more with the calibre of the contraction than with its cause. Of course, if the truth of these assertions be admitted, the accurate diagnosis of stric-

* Van Buren and Keyes, p. 151, *New York Medical Journal*, No 133, p. 388.

† "In the hands of designing persons these instruments may be turned to most unworthy purposes. For men outside the ranks of our profession, or holding a questionable position on its outskirts, they are the very thing."—Sir Henry Thompson.

tures in the sub-pubic region becomes, in many cases, exceedingly difficult. In practice, when the patient is possessed of a reasonable amount of intelligence, I place great weight upon the amount of pain which is excited by the introduction of the bougie. If at this point the patient simply complains of the peculiar sensations which always attend the passage of an instrument through the deep urethra, and not of severe pain, I usually dismiss the idea of stricture in this region. If, however, actual and severe pain is caused by the presence of a bougie, and a careful examination fails to reveal any coarctation in the anterior part of the canal, I consider it indicative of some abnormal condition—not necessarily stricture—of the deep urethra, and proceed to treat it by the introduction of full-sized sounds.*

This method is, of course, inexact, as the results vary with the susceptibility to pain of the individual; but I have usually found it sufficiently reliable.

Another frequent source of error is a spasmodic contraction of the urethral muscles around the bulb of the instrument, conveying to the hand a sensation which is very deceptive and is often almost undistinguishable from that imparted by a true stricture. This tendency to spasm will frequently be found associated with a contracted meatus, or with a stricture in the anterior portion of the canal.

It generally disappears during profound anæsthesia, and etherization may consequently serve as a means of clearing up doubtful cases.

The distinction between a spasmodic and an organic stricture may often be made from the quivering irregular manner in which the instrument is held when its stoppage is due to spasm, and still oftener by the fact that the obstruction is not at a definite point, but will be found at different portions of the urethra in different examinations. These qualities, however, do not invariably obtain in spasmodic stricture. The following case will serve to illustrate at once the deceptive symptoms which may accompany this form of stricture, its dependence upon true stricture situated at or near the meatus, and the very serious consequences of this latter form of contraction, even when of large calibre:

W. S., a gentleman æt. 27, strong, robust, weight one hundred and seventy pounds, complained to me of deep-seated lumbar pains, which he believed to be due to Bright's disease. A careful examination of his urine showed his kidneys to be in an entirely healthy condition. As there was a history of two or three antecedent attacks of gonorrhœa, I persuaded him to submit to a urethral examination. A dense cartilaginous stricture was found, situated almost directly at the meatus, and admitting a No. 16 bulbous bougie. This instrument was passed into the bladder and slowly withdrawn, two points of contraction being passed, one at three and a half, the other at five and a half inches from the meatus; the procedure and measurement were repeated with the same bougie, and then with a No. 18, with identical results. A No. 21 steel sound was then passed through the first stricture, but, as it gave some pain at this point, was withdrawn. A No. 19 passed by its own weight and without any resistance directly into the bladder. The instant, however, that the point of the instrument had traversed the prostatic urethra the patient became violently convulsed, the head was thrown back and held there rigidly, the eyes were rolled upwards so that only the whites were discernible. General convulsive movements of the most violent character occurred. There was slight frothing at the mouth, with marked opisthotonos, great lowering of temperature, cold perspiration, and a pulse of 50. After persisting for, perhaps, a minute or a minute and a half, the convulsive element disappeared, and the patient recovered consciousness, but in a state of profound weakness and depression. Half an hour later, after the administration of two ounces of brandy, the pulse was only 55, and the temperature 96.2°. Reaction followed very slowly, but in an hour or two the patient was able to walk home. There was no history of epilepsy in the family nor in the patient himself. Two

* I have found that in passing bulbous bougies in the cadaver I could often recognize the precise instant of their entering the bladder. On withdrawing the bougie it might be found impossible to tell by the touch when the bulb left the bladder, but on inserting it again it could be distinctly felt to hop over some obstacle at the internal meatus. Now, the length of the curved portion of the urethra in adults is but slightly variable, the differences in the length of the whole canal depending chiefly upon corresponding differences in the pendulous portion of the penis: consequently in the majority of cases we have an almost fixed distance between the mouth of the bladder and the triangular ligament. If, therefore, we could in the living as in the dead recognize definitely the moment at which the bulb entered the bladder, we could with equal definiteness fix the point at which we ought to find a normal contraction, and would therefore have a valuable aid to the detection of strictures in this region. Practically, however, I find that, although in some instances by great delicacy of manipulation it is possible to tell when the bulb passes into the bladder, in the majority of cases it is impossible to do so; occasionally, too, the sensations of the patient will be of value in the determination of this point, but they cannot be depended upon. I am making a series of measurements with this idea in mind, but have not yet reached a number which will justify much generalization, and feel doubtful as to the possibility of ever practically applying the facts elicited. The relation of the circumference of the internal meatus to that of the prostatic urethra, as shown by distending injections (Dr. H. B. Sands, *N. Y. Medical Journal*, March, 1876), would seem to be variable; of course when the meatus is the larger, no marked resistance would be offered by it.

days later I freely divided the stricture at the meatus with a probe-pointed bistoury, so that a No. 30 steel sound could be inserted without difficulty as far as the commencement of its shaft,—about three inches. This was done every day for a week or more, when a No. 24 bulbous was cautiously passed into the bladder, and was followed by a steel instrument of the same size, causing but little pain, and without producing any return of the alarming symptoms which have been mentioned. In two or three sittings this was increased to No. 28, which is nearly the normal calibre of the urethra (circumference of flaccid penis, a little less than three inches), and this instrument now slips easily and almost painlessly into the bladder. A bulbous sound No. 26 fails to detect the slightest contraction, with the exception of the one at the triangular ligament, and which my experience leads me to believe is rarely, if ever, absent. The pains which first caused the patient to seek professional advice have entirely disappeared. The case is still under treatment. I expect to increase the size of the bougie to No. 30, and to advise its occasional use for some time.

This case, like the other, cannot be considered a typical one, the ordinary symptoms being greatly exaggerated and intensified in both. But for that very reason they serve to illustrate with greater clearness the principles involved in the use of these instruments.

The production of convulsions by the gentle introduction of a bougie is itself an occurrence of such extreme rarity as to deserve mention, and, indeed, I have not been able to find recorded a precisely similar case. Syncopal attacks are comparatively common, and are sufficiently alarming. Nausea, painful and frequent micturition, urethral fever, cystitis, swelled testicle, or bubo are concomitants or sequelæ of catheterism with which every surgeon is familiar, and with the possibility of which he usually acquaints his patient; but general convulsions with such profound depression of all the vital forces is an accident which has hardly been considered as within the range of possibilities, and should serve as a salutary warning against the careless or unnecessary use of urethral instruments.

A source of error in the use of bulbous bougies, to which I shall merely allude as altogether inexcusable, is a malposition of the penis at the time the exploration is made. Any twisting or doubling of the organ upon its long axis will of course give rise to a sense of resistance during the withdrawal of the bougie; but, if the penis is gently extended, and is held with the dorsum facing the abdominal wall and parallel with it, no such mistake can be made; and, so far as the pendulous portion of the urethra is concerned, the instrument may be considered to be of great use.

The object of this paper will have been attained if it assists in demonstrating:

1st. The existence and pathological importance of strictures of extreme calibre.

2d. The absolute necessity for the intelligent use of bulbous bougies in the diagnosis of such strictures.

3d. The resistance offered to the withdrawal of bulbous bougies in normal urethræ by a constriction at or near the deep layer of perineal fascia.

4th. The possible occurrence of a spasmodic constriction almost undistinguishable from an organic stricture.

5th. The occasional dependence of such spasms upon coarctations of large calibre situated in the anterior portion of the canal.

222 South Sixteenth Street.

NOTES OF HOSPITAL PRACTICE.

PENNSYLVANIA HOSPITAL.

SERVICE OF DR. R. J. LEVIS.

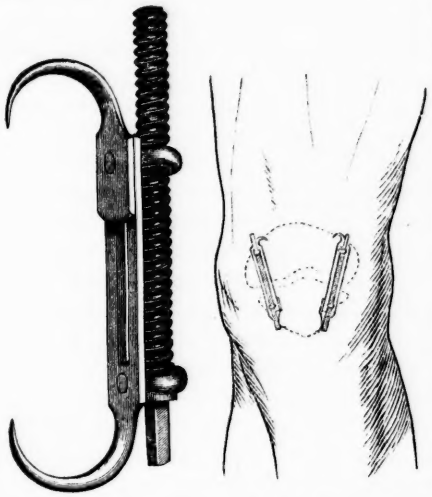
Reported by John B. Roberts, M.D.

FRACTURE OF THE PATELLA TREATED WITH A MODIFICATION OF MALGAIGNE'S HOOKS.

Mary J. was admitted with transverse fracture of the left patella, the fragments of which were separated by a distance of three-quarters of an inch. After elevation and anodyne lotions had been employed for three days, she was etherized, and Dr. Levis's modification of Malgaigne's hooks applied to bring the pieces of bone into accurate approximation. The modification consists simply in having two pairs of single hooks instead of the pair of double hooks usually seen. By this change the bone is more firmly and accurately held, because on account of the irregular surface of the patella it is difficult to make the double hooks sit accurately: they are like a four-legged stool on an uneven floor. After the hooks had been adjusted and firmly screwed together, carbolized oil was applied and the leg placed on an inclined plane.

The hooks gave accurate apposition of the fragments, and were kept in position a

little over five weeks; after their removal the same dressing was continued to the small ulcerated points, and the limb kept extended. The two patellæ were accurately measured, and found to be of the same length. She was soon allowed to walk with a posterior straight splint, and passive motion was instituted when this was discarded.



At the present time, which is four months since the receipt of injury, the leg can be flexed to a right angle; and the patient, though she still walks somewhat lame, is rapidly gaining perfect control of the limb. With the finger there can be felt a slight ridge at the point of fracture, but the two patellæ seem to be of the same length.

TRANSLATIONS.

THERAPEUTIC USES OF THYMOL (*Giornale di Medicina Militare*, Rome, December, 1876; from *Virchow's Archiv*).—In an article on thymol as an antiseptic and anti-fermentative, Lewin ascribes the following properties to the article in question. In a solution of one-tenth per cent., thymol arrests the fermentation of sugar, and in relatively small quantities retards the putrefaction of organic substances. If a solution of thymol is applied to the skin of a frog, there is induced a paralysis of the peripheral extremities of the sensory nerves of the skin, which anæsthesia is followed by an affection of the subjacent parts or muscles. If the agent be injected hypodermically, the same local symptoms occur without any involvement of the central nerves. If applied directly to the heart, this organ acts like the other muscles and remains paralyzed. In warm-blooded animals, however, whatever the dose, it does not produce when applied to the skin the same effect as in the frog: 120 grains of a one-tenth per cent. solution were administered to a rabbit without relative symptoms manifesting themselves. The appetite of the animal remained normal, and diuresis was produced.

In the medication of solutions, thymol acts as no other of the antiseptics hitherto thus employed. It produces in a short time cessation of vomiting in cases of abnormal fermentation in the stomach. In diphtheria and similar diseases there is reason to believe that thymol will become of great utility; and on account of its astringent action it can be employed to counteract hypersecretion of mucus. Internally, Lewin used a solution of 0.8 to 1 part in 1000 parts of water; externally the same, or 2 to 4 grains, with alcohol and water to make 1000 grains. J. B. R.

THE USE OF INDIA-RUBBER CLOTH IN SKIN DISEASES (*Giornale di Medicina Militare*, Rome, December, 1876).—This dressing fell into discredit because it was employed in moist eczemas and eczema rubrum, where the exudation increased and great exfoliation resulted. It is most advantageous in those processes accompanied by dry skin, in which evaporation is diminished by the use of the cloth; and so also in diseases presenting an excessive alteration of the epidermis, as psoriasis, the last stages of eczema, etc. Besides, it acts as an adjunct to other curative means, as, for example, in using tar inunctions. The patient uses the ointment at night and can in the morning after washing apply the gum cloth; by which means the perspiration keeps the skin moist under the dressing and the patient can attend to his customary vocation. In some forms of psoriasis the cloth alone is sufficient, as also in some diseases resulting from deficient secretion of the sebaceous glands. In cases of xeroderma a form of chronic urticaria passing into prurigo is apt to result from the treatment; so that though conquering the xeroderma we are in danger of producing an intractable itching.

J. B. R.

PHILADELPHIA
MEDICAL TIMES.

PHILADELPHIA, MAY 26, 1877.

EDITORIAL.

MEDICAL EDUCATION AT THE UNIVERSITY OF PENNSYLVANIA.

DURING the last twenty years there has been an enormous amount of writing concerning medical education in these United States: nevertheless the standard of graduation south of New England has been steadily lowered, and, to our thinking, the courses of lectures rendered more unsatisfactory. Much new matter has indeed been added to the curricula, the bait of clinical instruction has been alluringly spread; but the effect has been evil, because the attempt has been simply to pour into vessels already overfull. Formerly students went from their two winter courses with a well-laid foundation of principles upon which to build. At present the whole of the vastly enlarged science and art of medicine is hurriedly planed out in five months, and with this green unseasoned lumber the student throws together a Centennial shanty-palace,—no part of it well and permanently built, none finished and perfected.

Feeling deeply the evil, the Board of Trustees of the University of Pennsylvania have, after a discussion lasting over several years, resolved to reorganize at once the Medical Department. Although the details of the scheme have not yet been worked out, sufficient has been done to indicate the headlands of the plan. As is well known, the regulation number of professors in medical colleges is seven, and the period of study two sessions of five months each. The first alteration made by the University authorities consisted in increasing the number of professors from seven to eleven. This was carried out last year. These chairs and their occupants are as follows:

Clinical Surgery, vacant.
Clinical Medicine, Dr. Wm. Pepper.
Clinical Obstetrics and Gynæcology, Dr. Wm. Goodell.
Pathology, Dr. James Tyson.

The fundamental changes shadowed forth in the formation of these professorships are now consummated in the *compulsory attendance of the student upon three courses of lectures, and the arrangement of the lectures upon a graded plan.*

This much having been decided upon, the question at once suggested itself, How long shall each course of lectures be? Undoubtedly it would be theoretically much better if the session lasted eight instead of five months; and the first impulse was to lengthen the course; but, as we think, wiser counsels have prevailed, and the five months' winter term is to be preserved; we say wiser counsels, because it is perfectly possible to go too fast and to endanger permanence by a too rapid building. The step from the requirement of two to that of three winters is a really enormous one; and it would be very foolish to proceed further before it is seen whether the profession will support this advance. Further, a very elaborate and extensive course of spring lectures both upon scientific and practical subjects has been established and made free to the students of the winter course, so that those who desire it can most profitably spend their whole year in the city.

As already intimated, the details of the plan have not been worked out; but we think we are justified in stating some things that will not be, and some things that will be, done.

In the first place, the plan of instruction followed out at Harvard will not be copied. Much as we respect the gentlemen who had the hardihood and the wisdom to act as the pioneers in medical education, and earnestly as we have supported them, we

have always thought that the method adopted at Harvard smacks too much of the school-house.

Professors in medical colleges are, or ought to be, not pedagogues, but the foremost men of their age and country; the students are, or ought to be, not boys, but earnest men, steadfastly set upon fitting themselves for the most responsible positions on earth. It is a sheer waste of talent and time, a misuse of valuable force, for such professors to hear lessons; and it is almost an insult to require the recitation at intervals of so many pages of a book from such students. Let the great teacher pour forth his own thoughts and knowledge, and students gather up his wisdom, in the amphitheatre. Could anything be more disheartening than to see a John Hunter turned schoolmaster?

It may be answered that the medical college might have instructors to hear the recitations, and leave the professor to his lecture-room and laboratory. This certainly would be better than prostituting the professorial office; but the ordinary plan seems to us far better. What solid gain is obtained by having the instructor officially connected with the school we do not see. On the other hand, if the place is left open, and free trade, so to speak, allowed, the chance to prove and develop his teaching powers is afforded to every man. The "quiz room" becomes the nursery of professors. More than this, the ever-present competition must be an ever-present stimulus. The official instructor may fail to come up to his best, and yet retain sufficient popularity to prevent his dismissal or even the loss of his chances of rising in the school. In a quiz association composed of several men and harassed by a rival establishment, each member becomes a watchman over every other member, and the franchises of the class a barometer so sensitive and so public in its markings that it behooves every man to keep his own popularity as high as possible.

The base of every college is its young men, and its success depends at least in a considerable measure upon the hearty support and the numbers and personal strength of these men. To attempt to subvert the quizzes would, we think, be suicidal; and we are sure such a move is not contemplated.

The chief additions which will be made to the method now in use are the re-arrangement of lecturing, and the addition of laboratory work and of practical clinical instruction.

In regard to laboratory work, the University is supplied with an abundance of already partially furnished rooms; and it will be very easy to prepare for and accommodate as large a class as may present itself.

In regard to clinical advantages, there is scarcely a school in the United States that has as large a mass of clinical material at its very doors. There is the Hospital of the University itself, at which during the last year there were treated about six thousand patients. Across the street is the Philadelphia Hospital, with about one thousand beds, besides its lying-in and children's departments. During the winter seasons this hospital is crowded with patients, and a majority of its medical staff are University teachers. It will be seen that there is abundant opportunity for organizing the most complete and thorough system of bedside instruction; and we have no doubt this will be done. A portion of the second and a large part of the third year of the student will, we believe, be occupied in the hospital wards and the dispensary rooms. It is further proposed to add to the curriculum not only the didactic teaching of gynæcology, but also individual instruction upon actual cases.

The change in the University also touches the relations of the professors to the students, and to the Board of Trustees. The fees are hereafter to be paid to the

Board of Trustees, and salaries given to the professors. At least, such is the present expectation; although we hope finally the German plan of compensating professors partly by salary, partly by receipts from students, will be adopted.

An earnest effort is being made towards raising an endowment fund for the Medical Department; and the result so far has been so encouraging that when it was supplemented by the younger professors offering a personal sacrifice in favor of the new plan, the University authorities thought the time had come for the change. But it is impossible for men, however enthusiastic, to permanently work much below the market rate. The University salaries must before many years be put upon a uniform liberal scale, if the character of the Faculty is to be maintained. Much will no doubt be done by endowment, but fees from students must for a long time remain the chief source of income. The success or failure of the experiment rests, therefore, with the profession. If failure comes now, a quarter of a century must elapse before another trial; for the present experiment is made in the oldest and most famous school of the country, and under the most favorable auspices.

The alumni of the University will of course sustain their alma mater to their utmost, but for success more is wanted. Let the alumni of other schools for a time direct their students to the University and to Harvard. The success of these schools secured, the chief medical colleges of the country will not be slow to follow the lead.

ACCORDING to the *Buffalo Medical and Surgical Journal*, there are in that city to every doctor four hundred and twenty persons capable of paying, and the average medical "book income" from practice is four hundred and twenty dollars, one-half of which is usually not collected. The average man, woman, or child, to our thinking, pays more than a dollar a year for medical services. But, allowing this, there seem to be good grounds for the medical profession in Buffalo doing as the editor says: "Buffalo physicians are contented to strive to lay up treasure 'where moth and rust do not corrupt,' and have long since abandoned the pursuit of earthly treasure." There are a good many doctors in this city who, less philosophic, seem not to lay up treasure anywhere.

CORRESPONDENCE.

NEW YORK, May 8, 1877.

TO THE EDITOR OF THE PHILA. MEDICAL TIMES:

DEAR SIR:—Dr. E. C. Seguin has lately read an interesting paper before the Medical Journal Association, on "*The Use and Abuse of the Bromides.*" It was divided into two parts,—the first being devoted to a discussion of the characteristics of bromism, and the second to an exposition of his own methods of giving the bromides.

After a short historical sketch of the bromide of potassium, and a tribute to the labors of Locock and Brown-Séquard in connection with it, he went on to show how very extensive had been its employment of late years in all manner of affections. At first its use was largely empirical, but at the present day he thought this was becoming less and less the case. In regard to its *modus operandi*, there were two schools of opinion, one holding that its action was primarily on the vascular system, causing contraction of arterioles, and the other, that it acted directly upon nerve-elements. Personally he held to the latter view, which he thought was the only one that could satisfactorily explain all the phenomena accompanying its exhibition; but, however that might be, all observers were united in the conclusion that it had the general effect of lessening the irritability of nerve-centres, and especially of motor tracts.

In treating of the abuse of the bromides, Dr. Seguin made three subdivisions of the subject. First, he gave a detailed description of bromism in its milder and severer forms; secondly, he spoke of the latter as a complication in diagnosis; and, thirdly, he mentioned the medico-legal aspect of the subject.

In the milder cases of bromism some of the prominent symptoms mentioned were, the general debility, difficulty of utterance, amounting to a species of aphasia, acne, and the bromic breath. In a large class of nervous ailments not very clearly defined, some physicians, as well as druggists, were in the habit of recommending the patients to "take

a little bromide," and the practice thus instituted almost invariably resulted in a deterioration of the system. No physician, he thought, had a right to order medicines, and especially those of an injurious nature, in cases where they were not really called for. In the severer form of bromism he was inclined to believe that a central lesion might result. Among the symptoms mentioned were, paresis, a staggering gait, more or less incoherence of speech, acne, and sometimes rupia-like ulcers, diminished menses, and loss of virile power. The effects might be so marked as to amount to actual dementia. In concluding his description, Dr. Seguin pointed out the striking similarity between bromism and general paralysis of the insane.

Happily this severe form was exceedingly rare, except in epileptics who took at least thirty or forty grains of a bromide regularly every day.

In speaking of bromism as a complication in diagnosis, he mentioned two cases: one of Voisin, in which every authority but himself believed the patient to have general paralysis of the insane, but which made a rapid recovery as soon as the bromides were discontinued; and one of his own (seen in consultation), in which both the attending physician and a consulting oculist believed there was a central tumor of the brain. Dr. Seguin was of the opinion that only the *dura mater* was affected; and the result showed that the whole trouble was due to bromism.

He was not aware that the question of bromism had ever come up in medico-legal cases, but it was one liable to arise at any time. Among the points to be considered were—(1.) The responsibility of the physician in giving a medicine which has such a deleterious effect upon the patient both physically and mentally. For instance, a case had occurred in which an individual had fallen asleep in a railway-station while under the influence of bromide of potassium, and been robbed of four hundred dollars. (2.) The responsibility of the patient for criminal acts, such as taking articles and forgetting to pay for them, improper demonstrations through mistaken identity, etc. (3.) The responsibility of the patient in business transactions, making a will, etc. (4.) Death from bromism. There is a case on record in which excessive doses of the bromides previously ordered by a physician were kept up by the nurse, who paid no attention to the failing condition of the patient, until death finally resulted.

In this connection the question arises whether this method might not be employed by the criminally inclined in cases where it would be desirable for any reason to get rid of some individual.

Passing now to the second branch of his subject, Dr. Seguin spoke of his methods of using the bromides, especially in epilepsy. Here he used two solutions, the first formula being as follows:—℞ Potass. bromidi, ℥j; ammon. bromidi, ℥ss; aquæ, f℥vij. M.

The second is, ℞ Sodii bromidi, ℥j; ammon. bromidi, ℥ss; aquæ, f℥vii. M.

He has found it better to give the bromides thus in simple solution than in the form of elixirs or with syrups, and his plan is to give one teaspoonful of either of the above mixtures (they are both equally effective) in the morning and at noon, and two teaspoonfuls at night, in accordance with the practice of Brown-Séquard. The condition of the patient should be carefully studied from time to time, and the sensibility of the fauces watched, in order to keep up a mild bromism continuously for many months. Children bear much larger doses proportionally than adults, and from twenty to forty grains of the bromides may be given to mere infants with impunity. It is important that the remedy should be given largely diluted and upon an empty stomach. In epilepsy we are justified in producing bromism, but this is the only affection in which this is the case, unless, perhaps, in the opium habit, in which Dr. George M. Schweig, of this city, has found the bromides in large doses very efficient. We should never discontinue its use until the word "cure" can be pronounced. As Dr. Brown-Séquard says, the dose may be diminished during attacks of intercurrent affections, etc., but it ought never to be remitted. If this is done, all the good effects produced by a prolonged use of the remedy may be lost. The question then arises, How long should the bromides be kept up in epilepsy? According to Brown-Séquard and Voisin, for about three years; and this is to be regarded as the minimum period, the time occupied varying greatly in different cases. There are some patients who bear the remedy badly and are apparently but little benefited by it; but even in these it should be persevered in, as it may prevent the patient from dying in a fit. The bromides constitute by far the best means of treatment; but other agents are required in almost all cases, and especially to counteract any unpleasant effects that may arise from their use.

Thus, arsenic, quinia, strychnia, cream, cod-liver oil, stimulus, etc., may be employed; but the remedy which has proved in Dr. Seguin's hands of greater service than any as an adjunct to the bromides is belladonna. Dr. Seguin glanced hastily at several other affections besides epilepsy in which he had used the bromides with success, but gave it as his opinion that in many in which they had been employed they not only did no good, but were positively injurious.

At the last meeting of the County Medical Society, April 23, the subject for discussion was, "The Pharmacopœia of the United States: What Modifications are required in Order to meet the present Medical and Pharmaceutical Wants?" Among those who spoke were Drs.

Edward Curtis, Wm. H. Thomson, A. B. Crosby, and H. G. Piffard, and also several representatives of the College of Pharmacy. Most of the medical men, although acknowledging that they had not given the subject sufficient consideration to arrive at any positive conclusions, spoke in favor of the recommendations presented by Dr. Squibb in his late paper; while the latter earnestly opposed them. A resolution presented by Prof. Curtis to the effect that the Society was in favor of taking the Pharmacopœia out of its present hands and putting it in charge of the American Medical Association, and that at the approaching meeting of the latter its delegates should be instructed to vote for any plan by which this can be carried out, was referred to the Comitia Minora for future consideration.

At the last meeting of the Medical Journal Association, May 4, Dr. Eugene Dupuy made some remarks on the "*Therapeutics of Headache.*" He confined himself mainly to the subject of migraine, which he considered due to a nutritive irritation of that part of the encephalon in which the fifth pair of nerves has its origin. The membranes in the vicinity of the pons have either too great or too small a supply of blood, and, as a rule, the condition is one of impending congestion, sometimes bordering on an inflammatory process. For the treatment of the attack of headache he had found those means most successful which act in a reflex manner by causing an irritation of peripheral nerves. Thus, taking strong alcoholic liquors or snuff up into the nostrils was often of service; but the best agent for this purpose which he had found was carbonic acid gas, which should be forced into the nostril on the same side upon which the pain is located, and for whose ready manufacture he had devised an apparatus some years ago. Brown-Séquard had found that this gas controlled even the powerful convulsions occasioned by strychnia, and its inhalation for a minute was usually sufficient to completely dispel an attack of headache. One peculiar effect that it has is to cause a very sharp but momentary occipital pain at the insertion of the muscles, and in some cases it is necessary to keep up the inhalation for a minute and a half before this is produced. It is of no benefit in hysterical headaches, or in those due to the approach of one of the continued fevers. Of almost equal efficiency with the carbonic acid gas he has found the *potion de Rivière*, consisting of citric acid, bicarbonate of potassium, syrup, and water, to which a little bromide of potassium should be added. The third remedy which he suggested was a combination of aconitin, quinia, and tannin.

In the intervals between the attacks the treatment should, of course, vary according to the peculiar circumstances of each case, but in general he has found the following agents of the most service: arsenic, especially in combination with belladonna and ergot, or, better, ergotin, cannabis indica, with valerian or tannin, and aperients, particularly aloes, when it is not contra-indicated. A constitutional vice, such as the rheumatic, the gouty, or the syphilitic, should, of course, receive appropriate treatment when it is found to be present.

Dr. E. C. Seguin spoke of the great efficacy of *paulinia sorbilis* in warding off the attack of headache, especially if given when the patient first wakes in the morning with the premonitory symptoms of one. There were cases, however, in which nothing seemed of any service during the attack except the administration of strong narcotics; and in these he preferred the hypodermic injection of morphia and atropia combined. Where the attacks did not occur very frequently he did not think there was any danger of causing the formation of the opium habit in the patient. During the interval he had found cannabis indica by far the best remedy that he had employed, and he recommended that it should be employed in such a manner as to keep the patient more or less under its constitutional effect for a considerable period. In regard to the pathology of migraine he could not agree with Dr. Dupuy, and presented a number of reasons why he did not accept his views. He was of the opinion that it was a neurosis associated with vaso-motor disturbance, and that there was a well-defined resemblance between it and epilepsy.

Dr. Beard stated that he was inclined to accept the pathology advocated by Dr. Seguin, but that he could not believe that the seeming relation between this affection and epilepsy was anything more than a coincidence. This subject had been discussed in the Neurological Society, and this had been the general opinion expressed by the members at that time. Though the two affections were occasionally found in the same family, this was exceedingly rare. Dr. Beard rather startled some of the gentlemen present by asserting his belief that migraine was a blessing to those who suffer from it. He regarded it as a sort of safety-valve to the system, and recited instances in which its disappearance had been followed by serious organic trouble, especially of the spinal cord. The remedy which he had found the most useful of all in the attack was caffein, and in some instances in which it failed alone, he had employed it successfully in combination with carbonate of ammonia and the elixir of guarana, as suggested to him by Dr. Lincoln. After caffein, he had found the muriate of ammonia in fifteen or twenty grain doses the most useful agent. Electricity, as a rule, had not proved of much service in his hands, though in some particular instances he had used it with benefit in the form of galvanism.

A training-school for nurses is about to be opened in connection with the New York Hospital, and one feature of the course of

preparation will be that all the pupils shall spend one month each in the hospital kitchen and laundry. The new hospital is now in good working order, and a number of serious operations have already been performed in the amphitheatre by the surgical staff. Dr. Markoe, assisted by Dr. George Peters, recently made a very skilful exsection of the superior maxillary there, cutting out the bone completely to the back of the orbit. It is stated that the building has cost not less than $1,200,000. All its appointments are certainly of the most sumptuous and elegant character; but after a careful inspection of the institution one cannot resist the conclusion that an edifice of as really practical efficiency might have been put up for about one-third the price. $1,200,000 is, without doubt, rather an extravagant sum to expend on accommodations for two hundred patients; and it is to be regretted, we think, that a portion of this money at least was not devoted to the permanent endowment of a larger number of free beds.

As the hospital is now constituted, but very few free patients are admitted at all; so that it is probable that the usefulness of the institution would be considerably increased if a larger number of these were taken.

The following statistics of Bellevue Hospital for the year 1876 have been announced:

Number of patients remaining January 1, 1876 . . .		493
Number admitted during the year		5165
Total number treated		5658
Number discharged . . .	4313	
Number of deaths during the year	698	
Total . . .	5011	
Number remaining January 1, 1877		647

The expense of the hospital for the year was $110,568.79.

Out of 47,659 prisoners admitted to the Tombs during the year (of whom 30,926 were males, and 16,733 females) there were only six deaths.

A daughter of Dr. Hammond is about to be married to a gentleman who rejoices in the title of Marquis di Manfredi Lanza di Mercato Bianco. He is said to belong to one of the oldest families in Sicily, and to have served with distinction in the Neapolitan and Austrian armies. Since then, however, his career has been rather a romantic one. His family being, apparently, somewhat impecunious, he came to this country with the expectation of making a fortune, but, having failed to obtain employment as a civil engineer, he changed his views, and accepted a position as waiter at Delmonico's. It was while serving in this capacity at an entertainment at the house of the Rev. Dr. Howard Crosby, Chancellor of the University of New York, in which Delmonico had charge of the supper, that his identity was accidentally discovered. He was recommended by Mrs. Crosby to one of her business friends, and afterwards rose to a responsible position in the firm to which he belonged. Mrs. Crosby furthermore introduced him to Miss Hammond, but probably had little idea that a wedding would finally result from the meeting.

P. BRYNBERG PORTER.

GLEANINGS FROM EXCHANGES.

CLOSURE OF WOUND IN COMPOUND FRACTURE WITH TINCTURE OF BENZOIN (*The British Medical Journal*, March 24, 1877).—Dr. E. M. Sinclair reports the case of a boy, æt. 12, who had a severe fall, causing a simple fracture of both bones of the right forearm, about the junction of the lower and middle thirds, and a compound fracture of both bones of the left forearm, slightly lower down than the other. The upper extremities of both bones protruded through the wound, and a short piece of each had to be removed before coaptation could be obtained. A moderate degree of extension was secured by an extemporized splint having an "interruption" at the seat of fracture; the wound was then sealed with a piece of lint saturated with compound tincture of benzoin, covered by another piece folded into several layers, and also well saturated with the tincture. There was considerable restlessness for some days, and difficulty, as might be anticipated from the age of the patient, in keeping the *two* arms in proper position.

On the twelfth day there was a slight smell and suspicion of pus having formed under the lint. This was removed, and a large hard "scab" found over the site of the wound, with a very little pus escaping at one side on pressure. Carbolic acid oil was applied for a day or two, and then water-dressing, until the scab came off in pieces; and the wound, with the exception of a few granulations which had sprung up at one small spot, was cicatrized by the twenty-sixth day. There was never any regular secretion of pus, and a firm cicatrix transversely across the arm, about an inch and a half long and from one and a half to three lines in breadth, marked the position of the wound. When the boy was discharged, rotation was very limited in both hands, and a good deal of callus was formed at the seats of fracture, particularly in the left arm.

Three months and a half after discharge, rotation could be performed in both hands to nearly the normal extent. There was no appreciable difference in the length of the two arms on careful measurement. Callus could hardly be felt on either side.

CASE OF VESICAL CALCULUS THE NUCLEUS OF WHICH WAS A PIECE OF NECROSED BONE, REMOVED BY LITHOTOMY (*The British Medical Journal*, March 24, 1877).—Sir Henry Thompson communicated to a recent meeting of the Royal Medical and Chirurgical Society the case of a man, æt. 50, who suffered from stone in the bladder. A small orifice to the urethra and sensitive passages indicated lithotomy rather than lithotrity, and the former operation (lateral) was performed on October 30. After several unsuccessful attempts, the stone was grasped by a large forceps, and became crushed in them whilst holding it firmly for extraction. After extracting the large fragments, a large piece was found on a kind of shelf on the upper and back part of the bladder, of which a piece of necrosed bone was the nucleus. There was no vessel to tie; no tube was inserted; and the patient was in a very fair state. After the operation, it was learned that, thirteen years ago, the patient fell from a tree, eighteen or twenty feet, upon his right hip. An abscess formed, and two or three pieces of dead bone came away from the inside of the right thigh just below the adductor longus. He had no trouble till March, 1876, when bladder-symptoms began. The nucleus of necrosed bone weighed eight grains; the crushed fusible calculus (dry) weighed four hundred and thirty grains. The patient was now all but well. The case was another illustration of an unusual cause of calculus in the bladder, from necrosed bone detached from a neighboring portion of the skeleton; the first case having been reported to the Society by Sir Henry Thompson in 1866, who also had had a similar case at University College Hospital in 1872, in a lad fifteen years of age, who made a good recovery.

ECRASEURS (*The Lancet*, April 7, 1877).—The ease, rapidity, and immediate safety with which the tongue can be removed by the galvanic écraseur ought not to lead surgeons to overlook the serious drawbacks to this employment of a most valuable application of electrical science to surgery. These drawbacks are threefold. The great danger in the use of the hot wire arises from the putrefaction which takes place in the slough over the stump of the tongue. This poisons the air passing into the lungs, which then sets up acute and very fatal bronchitis, with, in some cases, a low form of pneumonia. We believe that most of the deaths after this operation are produced in this way. Another danger is septic absorption from the wound, causing cellulitis around the larynx, pharynx, and great vessels of the neck, with œdema glottidis; and when the slough separates, the patient runs a serious risk of secondary hemorrhage, which is not unfrequently fatal. These dangers are so great that many surgeons are returning to the use of the simple wire écraseur, which, although less convenient at the time of operation, is, on the whole, safer. The wire severs the tissues without leaving a thick slough to putrefy and slowly separate, and primary hemorrhage may be surely prevented by proceeding slowly and carefully. Rapidity in action is the danger, for the wire has to *crush*, not *cut*, its way through. If this precaution be taken, the operation should be as bloodless as if the hot wire had been used, and the consequent risks to the patient far less.

NOTES AND QUERIES.

WE have received from Dr. V. P. Gibney, of New York, a letter stating that the report of his paper in our New York letter of April 28 is very incorrect. Our New York correspondent is usually very accurate, and this is the first complaint we have had concerning the numerous papers he has analyzed. Without attempting to point out individual errors in the report, we would refer our readers to the full text, which is to be published in the *New York Medical Journal* for June and July.

THE AMERICAN NEUROLOGICAL ASSOCIATION.

The American Neurological Association will hold its third annual meeting in New York on Wednesday, June 6, and on the 7th and 8th.

OFFICIAL LIST

OF CHANGES OF STATIONS AND DUTIES OF OFFICERS OF THE MEDICAL DEPARTMENT U.S. ARMY FROM MAY 6, 1877, TO MAY 19, 1877, INCLUSIVE.

The following Medical Officers are ordered to represent the Medical Department at the annual meeting of the American Medical Association in Chicago on June 5 next:

SURGEON JOSEPH R. SMITH,
" W. C. SPENCER,
" J. J. WOODWARD,
" J. S. BILLINGS.

S. O. 106, A. G. O., May 18, 1877.

EDWARDS, L. A., SURGEON.—Leave of absence on Surgeon's certificate of disability further extended six months. S. O. 101, A. G. O., May 10, 1877.

BAILY, J. C., SURGEON.—To report to the Commanding General, Military Division of the Pacific, and Department of California, for duty in Department of California. S. O. 106, c. s., A. G. O.

MIDDLETON, J. V. D., SURGEON.—Relieved from duty in Department of Dakota, and ordered to Military Division of the Atlantic. S. O. 106, c. s., A. G. O.

BROOKE, JOHN, ASSISTANT-SURGEON.—Assigned to duty at Greenville, S. C. S. O. 92, Department of the South, May 16, 1877.

GARDNER, W. H., ASSISTANT-SURGEON.—Assigned to duty at Columbia, S. C. S. O. 92, c. s., Department of the South.

KINSMAN, J. H., ASSISTANT-SURGEON.—Relieved from duty in Department of Dakota, and ordered to Department of the Gulf. S. O. 106, c. s., A. G. O.

DE WITT, C., ASSISTANT-SURGEON.—Granted leave of absence for one month and twelve days. S. O. 98, A. G. O., May 7, 1877, and on its expiration to report to Commanding General of the Department of the Platte for assignment. S. O. 106, c. s., A. G. O.

KING, WM. H., ASSISTANT-SURGEON.—Assigned to duty at Cheyenne Agency, D. T. S. O. 55, Department of Dakota, May 3, 1877.

MAUS, L. M., ASSISTANT-SURGEON.—Relieved from duty in Department of the South, and ordered to Department of Dakota. S. O. 106, c. s., A. G. O.

ANDREWS, WM. C. C., ASSISTANT-SURGEON.—Drowned in Young's Bay, near Astoria, Oregon, April 19, 1877.